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CONTENTS.

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INAUGURAL ADDRESS BEFORE THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.[1]

UNDER the title 'Darwinism' it is convenient to designate the various work of biologists tending to establish, develop or modify Mr. Darwin's great theory of the origin of species. In looking back over twenty-five years it seems to me that we must say that the conclusions of Darwin as to the origin of species by the survival of selected races in the struggle for existence are more firmly established than ever. And this because there have been many attempts to gravely tamper with essential parts of the fabric as he left it, and even to substitute conceptions for those which he endeavored to establish, at variance with his conclusions. These attempts must, I think, be considered as having failed. A great deal of valuable work has been done in consequence; for honest criticism, based on observation and experiment, leads to further investigation, and is the legitimate and natural mode of increase of scientific knowledge. Amongst the attempts to seriously modify Darwin's doctrine may be cited that to assign a great and leading importance to Lamarck's theory as to the transmission by inheritance of newly 'acquired' characters, due chiefly to American paleontologists and to the venerated defender of such views, who has now closed his long life of great work, Mr. Herbert Spencer; that to attribute leading importance to the action of physiological con-

[1] Concluding part of the address given by Dr. E. Ray Lankester, at York, on August 1, 1906.

gruity and incongruity in selective breeding, which was put forward by another able writer and naturalist who has now passed from among us, Dr. George Romanes; further, the views of de Vries as to discontinuity in the origin of new species, supported by the valuable work of Mr. Bateson on discontinuous variation; and lastly, the attempt to assign a great and general importance to the facts ascertained many years ago by the Abbé Mendel as to the cross-breeding of varieties and the frequent production (in regard to certain characters in certain cases) of pure strains rather than of breeds combining the characters of both parents. On the other hand, we have the splendid series of observations and writings of August Weismann, who has in the opinion of the majority of those who study this subject rendered the Lamarckian theory of the origin and transmission of new characters altogether untenable, and has, besides, furnished a most instructive, if not finally conclusive, theory or mechanical scheme of the phenomena of heredity in his book, 'The Germ-plasm.' Professor Karl Pearson and the late Professor Weldon—the latter so early in life and so recently lost to us—have, with the finest courage and enthusiasm in the face of an enormous and difficult task, determined to bring the facts of variation and heredity into the solid form of statistical statement, and have organized, and largely advanced in, this branch of investigation, which they have termed 'Biometrics.' Many naturalists throughout the world have made it the main object of their collecting and breeding of insects, birds and plants, to test Darwin's generalizations and to expand the work of Wallace in the same direction. A delightful fact in this survey is that we find Mr. Alfred Russel Wallace (who fifty years ago conceived the same theory as that more fully stated by Darwin) actively working and publishing some of the most convincing and valuable works on Darwinism. He is still alive and not merely well, but pursuing his work with vigor and ability. It was chiefly through his researches on insects in South America and the Malay Islands that Mr. Wallace was led to the Darwinian theory; and there is no doubt that the study of insects, especially of butterflies, is still one of the most prolific fields in which new facts can be gathered in support of Darwin and new views on the subject tested. Prominent amongst naturalists in this line of research has been and is Edward Poulton, of Oxford, who has handed on to the study of entomology throughout the world the impetus of the Darwinian theory. I must here also name a writer who, though unknown in our laboratories and museums, seems to me to have rendered very valuable service in later years to the testing of Darwin's doctrines and to the bringing of a great class of organic phenomena within the cognizance of those naturalists who are especially occupied with the problems of variation and heredity. I mean Dr. Archdall Reid, who has with keen logic made use of the immense accumulation of material which is in the hands of medical men, and has pointed out the urgent importance of increased use by Darwinian investigators of the facts as to the variation and heredity of that unique animal, man, unique in his abundance, his reproductive activity, and his power of assisting his investigator by his own record. There are more observations about the variation and heredity of man and the conditions attendant upon individual instances than with regard to any other animal. Medical men need only to grasp clearly the questions at present under discussion in order to be able to furnish with ease data absolutely invaluable in quantity and quality. Dr. Archdall Reid has in two original books full of insight and new suggestions, the 'Present

Evolution of Man' and 'Principles of Heredity,' shown a new path for investigators to follow.

The attempt to resuscitate Lamarck's views on the inheritance of acquired[2] characters has been met not only by the demand for the production of experimental proof that such inheritance takes place, which has never been produced, but on Weismann's part by a demonstration that the reproductive cells of organisms are developed and set aside from the rest of the tissues at so early a period that it is extremely improbable that changes brought about in those other tissues by unaccustomed incident forces can be communicated to the germ-cells so as to make their appearance in the offspring by heredity. Apart from this, I have drawn attention to the fact that Lamarck's first and second laws (as he terms them) of heredity are contradictory the one of the other, and therefore may be dismissed. In 1894 I wrote:

> Normal conditions of environment have for many thousands of generations moulded the individuals of a given species of organism, and determined as each individual developed and grew 'responsive' quantities in its parts (characters); yet, as Lamarck tells us, and as we know, there is in every individual born a potentiality which has *not* been extinguished. Change the normal conditions of the species in the case of a young individual taken to-day from the site where for thousands of generations its ancestors have responded in a perfectly defined way to the normal and defined conditions of environment; reduce the daily or the seasonal amount of solar radiation to which the individual is exposed; or remove the aqueous vapor from the atmosphere; or alter the chemical composition of the pabulum accessible; or force the individual to previously unaccustomed muscular effort or to new pressures and strains; and (as Lamarck bids us observe), in spite of all the long-continued response to the earlier normal specific conditions, the innate congenital potentiality shows itself. The individual under the new quantities of environing agencies shows *new* responsive quantities in those parts of its structure concerned, new or *acquired* characters.
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> So far, so good. What Lamarck next asks us to accept, as his 'second law,' seems not only to lack the support of experimental proof, but to be inconsistent with what has just preceded it. The new character which is *ex hypothesi*, as was the old character (length, breadth, weight of a part) which it has replaced—a response to environment, a particular moulding or manipulation by incident forces of the potential congenital quality of the race—is, according to Lamarck, all of a sudden raised to extraordinary powers. The new or freshly acquired character is declared by Lamarck and his adherents to be capable of transmission by generation; that is to say, it alters the potential character of the species. It is no longer a merely responsive or reactive character, determined quantitatively by quantitative conditions of the environment, but becomes fixed and incorporated in the potential of the race, so as to persist when other quantitative external conditions are substituted for those which originally determined it. In opposition to Lamarck, one must urge, in the first place, that this thing has never been shown experimentally to occur; and in the second place, that there is no ground for holding its occurrence to be probable, but, on the contrary, strong reason for holding it to be improbable. Since the old character (length, breadth, weight) had not become fixed and congenital after many thousands of successive generations of individuals had developed it in response to environment, but gave place to a new character when new conditions operated on an individual (Lamarck's first law), why should we suppose that the new character is likely to become fixed after a much shorter time of responsive existence, or to escape the operation of the first law? Clearly there is no reason (so far as Lamarck's statement goes) for any such supposition, and the two so-called laws of Lamarck are at variance with one another.

In its most condensed form my argument has been stated thus by Professor Poulton: Lamarck's "first law assumes that a past history of indefinite duration is powerless to create a bias by which the present can be controlled; while the second assumes that the brief history of the present can readily raise a bias to control the future" (*Nature*, Vol. LI., 1894, p. 127).

[2] I use the term 'acquired' without prejudice in the sense given to that word by Lamarck himself.

An important light is thrown on some facts which seem at first sight to favor the Lamarckian hypothesis by the consideration that, though an 'acquired' character is not transmitted to offspring as the consequence of the action of external agencies determining the 'acquirement,' yet the tendency to react exhibited by the parent *is* transmitted, and if the tendency is exceptionally great a false suggestion of a Lamarckian inheritance can readily result. This inheritance of 'variation in tendencies to react' has a wide application, and has led me to coin the word 'educability' as mentioned in the section of this address on psychology.

The principle of physiological selection advocated by Dr. Romanes does not seem to have caused much discussion, and has been unduly neglected by subsequent writers. It was ingenious, and was based on some interesting observations, but has failed to gain support.

The observations of de Vries—showing that in cultivated varieties of plants a new form will sometimes assert itself suddenly and attain a certain period of dominance, though not having been gradually brought into existence by a slow process of selection—have been considered by him, and by a good many naturalists, as indicating the way in which new species arise in nature. The suggestion is a valuable one if not very novel, but a great deal of observation will have to be made before it can be admitted as really having a wide bearing upon the origin of species. The same is true of those interesting observations which were first made by Mendel, and have been resuscitated and extended with great labor and ingenuity by recent workers, especially in this country by Bateson and his pupils. If it should prove to be true that varieties when crossed do not, in the course of eventual interbreeding, produce intermediate forms as hybrids, but that characters are either dominant or recessive, and that breeds result having pure unmixed characters—we should, in proportion as the Mendelian law is shown to apply to all tissues and organs and to a majority of organisms, have before us a very important and determining principle in all that relates to heredity and variation. It remains, however, to be shown how far the Mendelian phenomenon is general. And it is, of course, admitted on all sides that, even were the Mendelian phenomenon general and raised to the rank of a law of heredity, it would not be subversive of Mr. Darwin's generalizations, but probably tend to the more ready application of them to the explanation of many difficult cases of the structure and distribution of organisms.

Two general principles which Mr. Darwin fully recognized appear to me to deserve more consideration and more general application to the history of species than he had time to give to them, or than his followers have accorded to them. The first is the great principle of 'correlation of variation,' from which it follows that, while natural selection may be favoring some small and obscure change in an unseen group of cells—such as digestive, pigmentary or nervous cells, and that change a change of selective value—there may be, indeed often is, as we know, a correlated or accompanying change in a physiologically related part of far greater magnitude and prominence to the eye of the human onlooker. This accompanying or correlated character has no selective value, is not an adaptation—is, in fact, a necessary but useless by-product. A list of a few cases of this kind was given by Darwin, but it is most desirable that more should be established. For they enable us to understand how it is that specific characters, those seen and noted on the surface by systematists, are not in most cases adaptations of selective value. They also open a wide vista of

incipient and useless developments which may suddenly, in their turn, be seized upon by ever-watchful natural selection and raised to a high pitch of growth and function.

The second, somewhat but by no means altogether neglected, principle is that a good deal of the important variation in both plants and animals is not the variation of a minute part or confined to one organ, but has really an inner physiological basis, and may be a variation of a whole organic system or of a whole tissue expressing itself at several points and in several shapes. In fact, we should perhaps more generally conceive of variation as not so much the accomplishment and presentation of one little mark or difference in weight, length or color, as the expression of a *tendency to vary* in a given tissue or organ in a particular way. Thus we are prepared for the rapid extension and dominance of the variation if once it is favored by selective breeding. It seems to me that such cases as the complete disappearance of scales from the integument of some osseous fishes, or the possible retention of three or four scales out of some hundreds present in nearly allied forms, favor this mode of conceiving of variation. So also does the marked tendency to produce membranous expansions of the integument in the bats, not only between the digits and from the axilla, but from the ears and different regions of the face. Of course, the alternative hairy or smooth condition of the integuments both in plants and animals is a familiar instance in which a tendency extending over a large area is recognized as that which constitutes the variation. In smooth or hairy varieties we do not postulate an individual development of hairs subjected one by one to selection and survival or repression.

The study of the physiology of unhealthy, injured or diseased organisms is called pathology. It necessarily has an immense area of observation and is of transcending interest to mankind, who do not accept their diseases unresistingly and die as animals do, so purifying their race, but incessantly combat and fight disease, producing new and terrible forms of it by their wilful interference with the earlier rule of nature.

Our knowledge of disease has been enormously advanced in the last quarter of a century, and in an important degree our power of arresting it, by two great lines of study going on side by side and originated, not by medical men nor physiologists in the narrow technical sense, but by naturalists, a botanist and a zoologist. Ferdinand Cohn, professor of botany in Breslau, by his own researches and by personal training in his laboratory, gave to Robert Koch the start on his distinguished career as a bacteriologist. It is to Metchnikoff, the zoologist and embryologist, that we owe the doctrine of phagocytosis and the consequent theory of immunity now so widely accepted.

We must not forget that in this same period much of the immortal work of Pasteur on hydrophobia, of Behring and Roux on diphtheria, and of Ehrlich and many others, to whom the eternal gratitude of mankind is due, has been going on. It is only some fifteen years since Calmette showed that if cobra poison were introduced into the blood of a horse in less quantity than would cause death, the horse would tolerate with little disturbance after ten days a full dose, and then day after day an increasing dose, until the horse without any inconvenience received an injection of cobra poison large enough to kill thirty horses of its size. Some of the horse's blood being now withdrawn was found to contain a very active antidote to

cobra poison—what is called an antitoxin. The procedure and preparation of the antitoxin is practically the same as that previously adopted by Behring in the preparation of the antitoxin of diphtheria poison. Animals treated with injections of these antitoxins are immune to the poison itself when subsequently injected with it, or, if already suffering from the poison (as, for instance, by snake-bite), are readily shown by experiment to be rapidly cured by the injection of the appropriate antitoxin. This is, as all will admit, an intensely interesting bit of biology. The explanation of the formation of the antitoxin in the blood and its mode of antagonizing the poison is not easy. It seems that the antitoxin is undoubtedly formed from the corresponding toxin or poison, and that the antagonism can be best understood as a chemical reaction by which the complex molecule of the poison is upset, or effectively modified.

The remarkable development of Metchnikoff's doctrine of phagocytosis during the past quarter of a century is certainly one of the characteristic features of the activity of biological science in that period. At first ridiculed as 'Metchnikoffism,' it has now won the support of its former adversaries.

For a long time the ideal of hygienists has been to preserve man from all contact with the germs of infection, to destroy them and destroy the animals conveying them, such as rats, mosquitoes and other flies. But it has now been borne in upon us that, useful as such attempts are, and great as is the improvement in human conditions which can thus be effected, yet we can not hope for any really complete or satisfactory realization of the ideal of escape from contact with infective germs. The task is beyond human powers. The conviction has now been arrived at that, while we must take every precaution to diminish infection, yet our ultimate safety must come from within—namely, from the activity, the trained, stimulated and carefully guarded activity, of those wonderful colorless amœba-like corpuscles whose use was so long unrecognized, but has now been made clear by the patiently continued experiments and arguments of Metchnikoff, who has named them 'phagocytes.' The doctrine of the activity and immense importance of these corpuscles of the living body which form part of the all-pervading connective tissues and float also in the blood, is in its nature and inception opposed to what are called the 'humoral' and 'vitalistic' theories of resistance to infection. Of this kind were the beliefs that the *liquids* of the living body have an inherent and somewhat vague power of resisting infective germs, and even that the mere living quality of the tissues was in some unknown way antagonistic to foreign intrusive disease-germs.

The first eighteen years of Metchnikoff's career, after his undergraduate course, were devoted to zoological and embryological investigations. He discovered many important facts, such as the alternation of generations in the parasitic worm of the frog's lung—*Ascaris nigrovenosa*—and the history of the growth from the egg of sponges and medusæ. In these latter researches he came into contact with the wonderfully active cells, or living corpuscles, which in many low forms of life can be seen by transparency in the living animal. He saw that these corpuscles (as was indeed already known) resemble the well-known amœba, and can take into their soft substance (protoplasm) at all parts of their surface any minute particles and digest them, thus destroying them. In a transparent water-flea Metchnikoff saw these amœba-like, colorless, floating blood-cor-

puscles swallowing and digesting the spores of a parasitic fungus which had attacked the water-fleas and was causing their death. He came to the conclusion that this is the chief, if not the whole, value of these corpuscles in higher as well as lower animals, in all of which they are very abundant. It was known that when a wound bringing in foreign matter is inflicted on a vertebrate animal the blood-vessels become gorged in the neighborhood and the colorless corpuscles escape through the walls of the vessels in crowds. Their business in so doing, Metchnikoff showed, is to eat up the foreign matter, and also to eat up and remove the dead, wounded tissue. He, therefore, called these white or colorless corpuscles 'phagocytes,' the eater-cells, and in his beautiful book on 'Inflammation,' published twenty years ago, proved the extreme importance of their activity. At the same time he had shown that they eat up intrusive bacteria and other germs; and his work for the last twenty years has mainly consisted in demonstrating that they are the chief, and probably the only, agents at work in either ridding the human body of an attack of disease-causing germs or in warding off even the commencement of an attack, so that the man or animal in which they are fully efficient is 'immune'—that is to say, can not be effectively attacked by disease-germs.

Disease-germs, bacteria or protozoa produce poisons which sometimes are too much for the phagocytes, poisoning them and so getting the upper hand. But, as Metchnikoff showed, the training of the phagocytes by weak doses of the poison of the disease-germ, or by weakened cultures of the disease-germ itself, brings about a power of resistance in the phagocytes to the germ's poison, and thus makes them capable of attacking the germs and keeping them at bay. Hence the value of inoculations.

The discussion and experiments arising from Metchnikoff's demonstrations have led to the discovery of the production by the phagocytes of certain exudations from their substance which have a most important effect in weakening the resistance of the intrusive bacteria and rendering them easy prey for the phagocyte. These are called 'sensitizers,' and have been largely studied. They may be introduced artificially into the blood and tissues so as to facilitate the work of the phagocytes, and no doubt it is a valuable remedial measure to make use of such sensitizers as a treatment. Sir A. E. Wright considers that such sensitizers are formed in the blood and tissues independently of the phagocytes, and has called them 'opsonins,' under which name he has made most valuable application of the method of injecting them into the body so as to facilitate the work of the phagocytes in devouring the hostile bacteria of various diseases. Each kind of disease-producing microbe has its own sensitizer or opsonin; hence there has been much careful research and experiment required in order to bring the discovery to practical use. Metchnikoff himself holds and quotes experiments to show that the 'opsonins' are actually produced by the phagocytes themselves. That this should be so is in accordance with some striking zoological facts, as I pointed out nearly twenty years ago. For the lowest multicellular animals provided with a digestive sac or gut, such as the polyps, have that sac lined by digestive cells which have the same amœboid character as 'phagocytes,' and actually digest to a large extent by swallowing or taking into their individual protoplasm raw particles of food. Such particles are enclosed in a temporary cavity, or vacuole, into which the cell-protoplasm secretes digestive ferment and other chemical agents. Now there is no doubt

that such digestive vacuoles may burst and so pour out into the polyp's stomach a digestive juice which will act on food particles outside the substance of the cells, and thus by the substitution of this process of outpouring of the secretion for that of ingestion of food particles into the cells we get the usual form of digestion by juices secreted into a digestive cavity. Now this being certainly the case in regard to the history of the original phagocytes lining the polyp's gut, it does not seem at all unlikely, but on the contrary in a higher degree probable, that the phagocytes of the blood and tissues should behave in the same way and pour out sensitizers and opsonins to paralyze and prepare their bacterial food. And the experiments of Metchnikoff's pupils and followers show that this is undoubtedly the case. Whether there is any great variety of and difference between 'sensitizers' and 'opsonins' is a matter which is still the subject of active experiment. Metchnikoff's conclusion, as recently stated in regard to the whole progress of this subject, is that the phagocytes in our bodies should be stimulated in their activity in order successfully to fight the germs of infection. Alcohol, opium, and even quinine, hinder the phagocytic action; they should, therefore, be entirely eschewed or used only with great caution where their other and valuable properties are urgently needed. It appears that the injection of blood-serum into the tissues of animals causes an increase in the number and activity of the phagocytes, and thus an increase in their resistance towards pathogenic germs. Thus Durham (who was a pioneer in his observations on the curious phenomena of the 'agglutination' of blood-corpuscles in relation to disease) was led to suggest the injection of sera during surgical operations, and experiments recently quoted by Metchnikoff seem to show that the suggestion was well founded. After years of opposition bravely met in the pure scientific spirit of renewed experiment and demonstration, Metchnikoff is at last able to say that the foundation-stone of the hygiene of the tissues—the thesis that our phagocytes are our arms of defence against infective germs—has been generally accepted.

Another feature of the progress of our knowledge of disease—as a scientific problem—is the recent recognition that minute animal parasites of that low degree of unicellular structure to which the name 'protozoa' is given, are the causes of serious and ravaging diseases, and that the minute algoid plants, the bacteria, are not alone in possession of this field of activity. It was Laveran—a French medical man—who, just about twenty-five years ago, discovered the minute animal organism in the red blood-corpuscles, which is the cause of malaria. Year by year ever since our knowledge of this terrible little parasite has increased. We now know many similar to, but not identical with it, living in the blood of birds, reptiles and frogs.

It is the great merit of Major Ross, formerly of the Indian Army Medical Staff, to have discovered, by most patient and persevering experiment, that the malaria parasite passes a part of its life in the spot-winged gnat or mosquito (*Anopheles*), not, as he had at first supposed, in the common gnat or mosquito (*Culex*), and that if we can get rid of spot-winged mosquitoes or avoid their attentions, or even only prevent them from sucking the blood of malarial patients, we can lessen, or even abolish, malaria.

This great discovery was followed by another as to the production of the deadly 'Nagana' horse and cattle disease in South Africa by a screw-like, minute animal parasite, the *Trypanosoma Brucei*. The Tsetze fly, which was already known in some way to produce this disease, was found by

Colonel David Bruce to do so by conveying by its bite the Trypanosoma from wild big-game animals, to the domesticated horses and cattle of the colonists. The discovery of the parasite and its relation to the fly and the disease was as beautiful a piece of scientific investigation as biologists have ever seen. A curious and very important fact was discovered by Bruce—namely, that the native big game (zebras, antelopes, and probably buffaloes), are *tolerant* of the parasite. The Trypanosoma grows and multiplies in their blood, but does not kill them or even injure them. It is only the unaccustomed introduced animals from Europe which are poisoned by the chemical excreta of the Trypanosomes and die in consequence. Hence the wild creatures—brought into a condition of tolerance by natural selection and the dying out of those susceptible to the poison—form a sort of 'reservoir' of deadly Trypanosomes for the Tsetze flies to carry into the blood of new-comers. The same phenomenon of 'reservoir-hosts' (as I have elsewhere called them) has since been observed in the case of malaria; the children of the native blacks in Africa and in other malarious regions are *tolerant* of the malarial parasite, as many as 80 per cent. of children under ten being found to be infected, and yet not suffering from the poison. This is not the same thing as the immunity which consists in *repulsion* or *destruction* of the parasite.

The Trypanosomes have acquired a terrible notoriety within the last four years, since another species, also carried by a Tsetze fly of another species, has been discovered by Castellani in cases of sleeping sickness in Uganda, and demonstrated by Colonel Bruce to be the cause of that awful disease. More than 200,000 natives of Uganda have died from it within the last five years. It is incurable, and, sad to relate, not only a certain number of European employees have succumbed to it in tropical Africa, but a brave young officer of the Army Medical Corps, Lieutenant Tulloch, has died from the disease acquired by him in the course of an investigation of this disease and its possible cure, which he was carrying out, in association with other men of science, on the Victoria Nyanza Lake in Central Africa. Lieutenant Tulloch was sent out to this investigation by the Royal Society of London, and I will venture to ask you to join that body in sympathy for his friends, and admiration for him and the other courageous men who risk their lives in the endeavor to arrest disease.

Trypanosomes are now being recognized in the most diverse regions of the world as the cause of disease—new horse diseases in South America, in North Africa, in the Philippines and East India are all traced to peculiar species of Trypanosome. Other allied forms are responsible for Delhi-sore, and certain peculiar Indian fevers of man. A peculiar and ultra-minute parasite of the blood cells causes Texas fever, and various African fevers deadly to cattle. In all these cases, as also in that of plague, the knowledge of the carrier of the disease, often a mite or acarus—in that of plague the flea of the rat—is extremely important, as well as the knowledge of reservoir-hosts when such exist.

The zoologist thus comes into closer touch than ever with the profession of medicine, and the time has arrived when the professional students of disease fully admit that they must bring to their great and hopeful task of abolishing the diseases of man the fullest aid from every branch of biological science. I need not say how great is the contentment of those who have long worked at apparently useless branches of science, in the belief that all knowledge is good, to find that the science they have cultivated

has become suddenly and urgently of the highest practical value.

I have not time to do more than mention here the effort that is being made by combined international research and cooperation to push further our knowledge of phthisis and of cancer, with a view to their destruction. It is only since our last meeting at York that the parasite of phthisis or tubercle has been made known; we may hope that it will not be long before we have similar knowledge as to cancer. Only eighteen months have elapsed since Fritz Schaudinn discovered the long-sought parasitic germ of syphilis, the *Spirochæta pallida*. As I write these words the sad news of Schaudinn's death at the age of thirty-five comes to me from his family at Hamburg—an irreparable loss.

Let me finally state, in relation to this study of disease, what is the simple fact—namely, that if the people of Britain wish to make an end of infective and other diseases they must take every possible means to discover capable investigators, and employ them for this purpose. To do this, far more money is required than is at present spent in that direction. It is necessary, if we are to do our utmost, to spend a thousand pounds of public money on this task where we now spend one pound. It would be reasonable and wise to expend ten million pounds a year of our revenues on the investigation and attempt to destroy disease. Actually, what is so spent is a mere nothing, a few thousands a year. Meanwhile our people are dying by thousands of preventable disease.

Whilst I have been able, though in a very fragmentary and incomplete way, to indicate the satisfactory and, indeed, the wonderful progress of science since this association last met in York, so far as the making of new knowledge is concerned, I am sorry to say that there is by no means a corresponding 'advancement' of science in that signification of the word which implies the increase of the influence of science in the life of the community, the increase of the support given to it, and of the desire to aid in its progress, to discover and then to encourage and reward those who are specially fitted to increase scientific knowledge, and to bring it to bear so as to promote the welfare of the community. I am speaking on a privileged occasion to a body of men who are met together for the advancement of science, and I claim the right to say to them, without offence to the representatives of institutions which I criticize, what is in my mind.

It is, unfortunately, true that the successive political administrators of the affairs of this country, as well as the permanent officials, are altogether unaware to-day, as they were twenty-five years ago, of the vital importance of that knowledge which we call science, and of the urgent need for making use of it in a variety of public affairs. Whole departments of government in which scientific knowledge is the one thing needful are carried on by ministers, permanent secretaries, assistant secretaries and clerks who are wholly ignorant of science, and naturally enough dislike it since it can not be used by them, and is in many instances the condemnation of their official employment. Such officials are, of course, not to be blamed, but rather the general indifference of the public to the unreasonable way in which its interests are neglected.

A difficult feature in treating of this subject is that when one mentions the fact that ministers of state and the officials of the public service are not acquainted with science, and do not even profess to understand its results or their importance, one's statement of this very obvious and notorious fact is apt to be regarded as a personal offence. It is difficult to see wherein the

offence lies, for no one seeks to blame these officials for a condition of things which is traditional and frankly admitted.

This is really a very serious matter for the British Association for the Advancement of Science to consider and deal with. We represent a line of activity, a group of professions which are, in our opinion, of vital importance to the well-being of the nation. We know that those interests which we value so highly are not merely ignored and neglected, but are actually treated as of no account or as non-existent by the old-established class of politicians and administrators. It is not too much to say that there is a natural fear and dislike of scientific knowledge on the part of a large proportion of the persons who are devoid of it, and who would cease to hold, or never have held, the positions of authority or emolument which they now occupy, were scientific knowledge of the matters with which they undertake to deal required of them. This is a thorny subject, and one in which, however much one may endeavor to speak in general terms, it is difficult to avoid causing personal annoyance. Yet it seems to me one which, believing as I do that it is of most urgent importance, it is my duty as your president to press upon the attention of the members of the British Association. Probably an inquiry into and discussion of the neglect of science and the questionable treatment of scientific men by the administrative departments of government would be more appropriate to a committee appointed by the council of the association for this purpose than to the presidential address.

At the same time, I think the present occasion is one on which attention should be drawn in general terms to the fact that science is not gaining 'advancement' in public and official consideration and support. The reason is, I think, to be found in the defective education, both at school and university, of our governing class, as well as in a racial dislike among all classes to the establishment and support by public funds of posts which the average man may not expect to succeed by popular clamor or class privilege in gaining for himself—posts which must be held by men of special training and mental gifts. Whatever the reason for the neglect, the only remedy which we can possibly apply is that of improved education for the upper classes, and the continued effort to spread a knowledge of the results of science and a love for it amongst all members of the community. If members of the British Association took this matter seriously to heart they might do a great deal by insisting that their sons, and their daughters too, should have reasonable instruction in science both at school and college. They could, by their own initiative and example, do a good deal to put an end to the trifling with classical literature and the absorption in athletics which is considered by too many schoolmasters as that which the British parent desires as the education of his children.

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It is more agreeable to me not to dwell further on the comparative failure of science to gain increased influence and support in this country, but to mention to you some instances on the other side of the account. As long ago as 1842 the British Association took over and developed an observatory in the Deer Park at Kew, which was placed at the disposal of the association by Her Majesty the Queen. Until 1871 the association spent annually a large part of its income—as much in later years as £600 a year—in carrying on the work of the Kew Observatory, consisting of magnetic, meteorological and physical observations. In 1871 the association handed over the observatory to the Royal Society, which

had received an endowment of £10,000 from Mr. Gassiot for its maintenance, and had further devoted to that purpose considerable sums from its own donation fund and government grant. Further aid for it was also received from private sources. From this observatory at last has sprung, in the beginning of the present century, the National Physical Laboratory in Bushey Park, a fine and efficient scientific institution, built and supported by grants from the state, and managed by a committee of really devoted men of science who are largely representatives of the Royal Society. In addition to the value of the site and buildings occupied by the National Physical Laboratory, the government has contributed altogether £34,000 to the capital expenditure on new buildings, fittings and apparatus, and has further assigned a grant of £6,000 a year to the working of the laboratory. This institution all men of science are truly glad to have gained from the state, and they will remember with gratitude the statesmen—the late Marquis of Salisbury, the Right Hon. Arthur J. Balfour, Mr. Haldane and others—as well as their own leaders—Lord Rayleigh, Sir William Huggins and the active body of physicists in the Royal Society—who have carried this enterprise to completion. The British Association has every reason to be proud of its share in early days in nursing the germ at Kew which has at length expanded into this splendid national institution.

I may mention also another institution which, during the past quarter of a century, has come into existence and received, originally through the influence of the late Lord Playfair (one of the few men of science who have ever occupied the position of a minister of the crown), and later by the influence of the Right Hon. Joseph Chamberlain, a subsidy of £1,000 a year from the government and a contribution of £5,000 towards its initial expenses. This is the Marine Biological Association, which has a laboratory at Plymouth, and has lately expended a special annual grant, at the spontaneous invitation of His Majesty's Treasury, in conducting an investigation of the North Sea in accordance with an international scheme devised by a central committee of scientific experts. This scheme has for its purpose the gaining such knowledge of the North Sea and its inhabitants as shall be useful in dealing practically and by legislation with the great fisheries of that area. You will, perhaps, not be surprised to hear that there are persons in high positions who, though admittedly unacquainted with the scientific questions at issue or the proper manner of solving them, are discontented with the action of the government in entrusting the expenditure of public money to a body of scientific men who give their services, without reward or thanks, to carrying out the purposes of the international inquiry. Strange criticisms are offered by these malcontents in regard to the work done in the international exploration of the North Sea, and a desire is expressed to secure the money for expenditure by a less scientific agency. I do not hesitate to say here that the results obtained by the Marine Biological Association are of great value and interest, and, if properly continued and put to practical application, are likely to benefit very greatly the fishery industry; on the other hand, if the work is cut short or entrusted to incompetent hands it will no doubt be the case that what has already been done will lose its value—that is to say, will have been wasted. There is imminent danger of this perversion of the funds assigned to this scientific investigation taking place. There is no guarantee for the continuance of any funds or offices assigned to science in one generation by the officials of the next. The mastership of

the mint held by Isaac Newton, and finally by Thomas Graham, has been abolished and its salary appropriated by non-scientific officials. Only a few years ago it was with great difficulty that the government of the day was prevented from assigning the directorship of Kew Gardens to a young man of influence devoid of all knowledge of botany!

One of the most solid tests of the esteem and value attached to scientific progress by the community is the dedication of large sums of money to scientific purposes by its wealthier members. We know that in the United States such gifts are not infrequent; they are rare in this country. It is, therefore, with especial pleasure that I call your attention to a great gift to science in this country made only a few years ago. Lord Iveagh has endowed the Lister Institute, for researches in connection with the prevention of disease, with no less a sum than a quarter of a million pounds sterling. This is the largest gift ever made to science in this country, and will be productive of great benefit to humanity. The Lister Institute took its origin in the surplus of a fund raised by Sir James Whitehead when Lord Mayor, some sixteen years ago, for the purpose of making a gift to the Pasteur Institute in Paris, where many English patients had been treated, without charge, after being bitten by rabid dogs. Three thousand pounds was sent to M. Pasteur, and the surplus of a few hundred pounds was made the starting-point of a fund which grew, by one generous gift and another, until the Lister Institute on the Thames Embankment at Chelsea was set up on a site presented by that good and high-minded man, the late Duke of Westminster.

Many other noble gifts to scientific research have been made in this country during the period on which we are looking back. Let us be thankful for them, and admire the wise munificence of the donors. But none the less we must refuse to rely entirely on such liberality for the development of the army of science, which has to do battle for mankind against the obvious disabilities and sufferings which afflict us and can be removed by knowledge. The organization and finance of this army should be the care of the state.

It is a fact which many of us who have observed it regret very keenly, that there is to-day a less widespread interest than formerly in natural history and general science, outside the strictly professional arena of the school and university. The field naturalists among the squires and the country parsons seem nowadays not to be so numerous and active in their delightful pursuits as formerly, and the mechanics' institutes and lecture societies of the days of Lord Brougham have given place, to a very large extent, to musical performances, bioscopes and other entertainments, more diverting, but not really more capable of giving pleasure than those in which science was popularized. No doubt the organization and professional character of scientific work are to a large extent the cause of this falling-off in its attraction for amateurs. But perhaps that decadence is also due in some measure to the increased general demand for a kind of manufactured gaiety, readily sent out in these days of easy transport from the great centers of fashionable amusement to the provinces and rural districts.

In conclusion, I would say a word in reference to the associations of our place of meeting, the birthplace of our society. It seems to me not inappropriate that a society for the advancement of science should have taken its origin under the walls of York Minster, and that the clergy of the great cathedral should have stood by its cradle. It is not true that there is an essential antagonism between the scientific

spirit and what is called the religious sentiment. "Religion," said Bishop Creighton, "means the knowledge of our destiny and of the means of fulfilling it." We can say no more and no less of science. Men of science seek, in all reverence, to discover the Almighty, the Everlasting. They claim sympathy and friendship with those who, like themselves, have turned away from the more material struggles of human life, and have set their hearts and minds on the knowledge of the Eternal.

THE ITHACA MEETING OF THE AMERICAN CHEMICAL SOCIETY. II.

SECOND GENERAL SESSION.

At the second general session the following addresses were given:

Hydronitric Acid: L. M. DENNIS.

Recent Progress in Industrial Chemistry: J. D. PENNOCK.

The address first cites the rapid progress of the ammonia soda process in comparison with the Le Blanc, showing that from 1870 to 1903 the world's production of soda by the ammonia process increased from 2,600 to 1,150,000 tons per annum, and the Le Blanc process decreased from 447,000 in 1870 to 150,000 in 1903. The reason for the continuance of industrial life in the Le Blanc works is the income obtained from bleaching powder and sulphur products. The progress of the electrolytic process is then discussed and a statement is made that in 1905 considerable profit was made by at least two concerns in England on the manufacture of electrolytic caustic soda and bleaching powder. Comment was made on the development of the treatment of fatty acids by carbonate of soda in the manufacture of soap, rather than the old method of treating neutral fats with caustic soda and thereby making a considerable saving. It was predicted that the tendency in industrial operations to the use of fuel gas from coke ovens and producers will ultimately do away with the smoke nuisance. The utilization of peat for producer gas and in the making of crude paper are interesting developments along this line. The introduction of alcohol free from tax after January 1, 1907, will call for a denaturizing substance of some sort. Doubtless the United States will follow the German practise of using a 2.5 per cent. solution made up of four liters of wood alcohol and one liter of pyridine added to 100 liters of alcohol. The future source of suitable nitrogen for the soil was then discussed. First dealing with Dr. Frank's calcium cyanimide process, then with Birkland and Eydes fixation of atmospheric nitrogen, and with the immediate and more practical application of ammonium sulphate manufactured on a large scale by the by-product coke ovens. It was predicted that in about thirty years the supply of nitrate of soda will be exhausted. The source of nitrogen must then be furnished by one of the above described processes.

Some Problems of Biological Chemistry: WALDEMAR KOCH.

It was pointed out that it is possible to investigate a tissue from the point of view of a chemist without isolating definite chemical substances, by dividing the constituents into the following groups: proteids, carbohydrates, fats, lecithins, extractives, ash. These groups have a physiological significance and the distribution among them of definite substances or elements like phosphorus, which was especially discussed, can vary in some of these groups within wide limits under different physiological conditions. The possibility of determining the physiological value of the different combinations of phosphoric acid to the cell was illustrated by a number of examples.